any two points in monocular vision. Images upon different sides of the fovea in monocular perception never combine, and are never supposed to combine. Now, supposing C and D in the foveæ e and c' by convergence, and keeping in mind the fact that the temporal half of the right eye in binocular perception corresponds to the nasal half of the left in monocular perception, the images of A and B, while they fall in non-corresponding halves, occupy positions visually the same as if they fell upon non-corresponding halves in monocular perception, the temporal and nasal; and hence, superimposing L upon R, e' would fall as far from the fovea in the nasal half as a from the fovea in the temporal half of the left eye L: that is, the images of A and B, a and e', visually fall upon opposite sides of the fovea, and can no more combine than separate images in monocular perception.

The same general result is obtained if we combine C and D by diverging the eyes; that is, by focusing the eyes in the median line beyond the point E, or beyond the stereoscopic figures. The eyes are thus turned outwards, so that the fovea in each case must be shifted inward from c to d, and from c' to b'. Combination of C and D will thus be attained by intra-foveal images, — such as are intra-foveal while the point of fixation remained at E. But when d and b' are brought into their corresponding fovea, e and a' still remain intra-foveal at distances from the fovea equal to that between d and e, and a' and b'. By the same argumentation as before, it can be shown that the images of A and B, respectively e and a', cannot combine. Thus, being both intra-foveal, they fall upon points in the nasal halves of the two eyes. These are binocularly non-corresponding, and therefore monocularly complementary halves of the retina: hence falling upon e and a' in binocular vision is the same as occupying opposite sides of the fovea in monocular vision, and so combination will be impossible. This shows the importance of observing what is implied by the term 'disparate.' As long as we conceive the term in its binocular application, there would be some reason for supposing combination upon them under the circumstances described. But adjustment by convergence and divergence, the former for extra-foveal and the latter for intra-foveal images, requires us to think of 'disparate' in its monocular application; and in that case we must either deny the possibility of combination upon them, or abandon the whole theory which makes a nasal half of one eye correspond to a temporal half of the other; for, if 'disparate' points in monocular perception may admit of combination, a nasal half may correspond to a nasal half, and a temporal to a temporal half, of the retina. This has never been assumed to be possible.

Of course, 'intra-foveal' and 'extra-foveal' are used with reference to the vertical meridian, and not the horizontal meridian, as Fig. 4 would seem to imply. In the last figure A and B represent positions relative to the vertical meridian of any objects in the temporal halves of the retina, and hence they may be above or below the horizontal meridian in which they really lie, according to the inclination of the lines to the median plane. The modification for the nasal halves of the retina can be supplied by the reader. It is evident from this that this demonstration does not apply mathematically to Fig. 1, where the apparent fusion is of binocularly 'disparate' points, although, taken in the *total* sense for *localization*, it will apply. But it is combination, not localization, that we are discussing.

If the stereoscope is used to effect the combination, the perspective noticed in convergence with the naked eyes is reversed, and is identical with that effected by the divergent movement to produce combination. The reason for this may be briefly stated. The partition between the lenses lies in the median line, and hence cuts off the extra-foveal images entirely. Combination has therefore to be effected by the intra-foveal. With this statement of the conditions, the argument could be carried out as before.

But the reply to our position that stereoscopic combination upon 'disparate' points must be impossible, will be the very plain one that it contradicts the *facts* of actual vision; that we can actually see the combination to have taken place; and, since it cannot have been upon corresponding, it must have been upon 'disparate' points. There are two replies to this, and, in addition, an important fact which explains the apparent anomaly. In the first place, the demonstration is mainly intended to show that the phenomenon must be impossible if we still retain the ordinary theory in regard to the divisions of the retina and their functions. In the second place, experiment shows that our claim is correct: for, after long practice in combination by convergence or by divergence, those images which, according to construction, must fall upon disparate points, and which at first seemed to be single and to coincide, appear double until they are brought into the fovea. This indicates that they were never really fused into one. Why, then, is the fusion so apparent to vision? The answer is, that inhibition had suppressed such portions of one or both images contending for fusion, that the resultant, made up of complementary elements, appears as a single image. After considerable practice, the reflex and automatic tendency is weakened, and inhibition correspondingly decreases; so that the images which before seemed single appear double, as the law of disparate points requires.

Baltimore, Md., Jan. 4. J. H. HYSLOP.

Bacteriology as a Study in Schools.

THE subject of the study of bacteria, discussed by Professor Conn in a recent number of *Science* (xi. No. 257), is one which deserves more attention than it has attracted thus far, and I take the liberty of making a few suggestions which have presented themselves to an investigator rather than a teacher, but which may prove useful to the latter. Let us call the subject 'bacteriology' for convenience' sake, and drop the misleading expression 'germ-theory of disease,' which has had its day. We know, as positively as we know that the earth revolves on its axis, that certain diseases in man and animals are caused by the invasion and multiplication of bacterial parasites. There is no theory about this. The phrase is misleading, because it states that all disease may be due to germs, which is manifestly untrue.

There are several classes of students who would be greatly benefited by a careful study of bacteria in the laboratory.

1. Students of general biology and physiology would gain by a few simple experiments, readily performed, a very clear insight into the great metabolic activity of life in general, of bacteria in particular. It would be easy to demonstrate the formation of soluble ferments related to pepsin and diastase; the production of soluble and insoluble pigments, and the effect of re-agents upon them; the relation of vital activity to oxygen as expressed by aerobic and anaerobic germs; the effect of bacterial growth on various substances, such as blood serum, gelatin, and milk; the resistance of spores to high temperatures; the effect of disinfectants and antiseptics; the phenomena of phosphorescence, nitrification, and other equally interesting and instructive features of bacterial life. The habit of close observation and careful differentiation may be cultivated by the parallel study of two species as nearly alike as possible. All this, and more, can be done with bacteria obtainable at any time, from natural waters, from the soil, the digestive tract of mammals and other animals, from milk and various infusions. To impress the mind with the destructive effect of pathogenic forms, a rabbit, or mouse, or guinea-pig may be inoculated with some germ fatal to these animals, but harmless to man. Such a form, fatal to rabbits, is occasionally present in the mouth. The microscopic study of bacteria brings out facts of histo-chemistry, and features of the microscope itself hitherto scarcely known, which should be applied in ordinary histologic work.

2. There is another class of students who stand in need of such instruction. Much of the preparatory work of the student of medicine can and should be done at our higher institutions of learning. For instance, the admirable work done at Cornell University in preparing students for the study of medicine, of which I have personal knowledge, has always tended to push students into the front rank at the medical schools. These have no time to spare to teach students how to dissect well, how to study anatomy or to acquire the methods underlying histologic work and chemical analysis, nor have they the time to teach bacteriology. Yet no one should graduate in medicine to-day who does not know something about the secret working of this microscopic world, who cannot reason with it in his practice, or recognize the different forms when a diagnosis may be based upon them. Our biological laboratories may do much to help the medical schools in this direction. The physician will then be equipped with healthier ideas concerning the 'germ-theory;'

and the adverse opinion still expressed upon it by many, which may be safely called the opinion of ignorance, will soon be heard no more.

Another class needs some knowledge of bacterial life. This includes all, — the father, the mother, the teacher, the citizen. Whoever has charge of human life should know something of the nature of infection with its manifold ways, of the necessity of disinfection and the means within reach. Education in such subjects is the only means of strengthening our present lax and indifferent spirit with reference to the public health. For this third and largest class a brief course of lectures, with demonstrations that will impress firmly the reality of the vital force inherent in bacteria, would be amply sufficient. What is needed is a certain attitude, an intelligent receptivity of the younger generations which will be favorable to all proper measures for the protection of public and private health, and which will promote in every way the study of the laws that underlie it.

The teaching of hygiene is taking root rapidly and firmly in the continental universities, and bacteriology is intrusted to such chairs. Our own higher institutions are beginning to realize the need of such instruction. As yet we have not gotten far beyond muscle, but that is a very good beginning. Bacteriology, though linked to hygiene as a branch of study, should, for the time being, find its place without difficulty in the biological laboratory.

THEOBALD SMITH.

Washington, D.C., Jan. 23.

Queries.

25. TREE TEMPERATURES. — In speaking with two farmers, each of more than ordinary intelligence, one last winter and another this, on the subject of temperature, they asserted that a thermometer hung against the trunk of a living tree of any size would not register as low as if suspended (as one made the observation) from a wire clothes-line, and the other from a pine post. Is this a fact?

D. LICHTY.

Rockford, Ill., Jan. 23.

26. THE EARTH'S ROTATION AS AFFECTING RAILWAY-TRAINS. — In Maury's 'Physical Geography of the Sea,' edition 1855, p. 39, paragraph 43 reads as follows: "Take for illustration a railroad that runs north and south. It is well known to engineers that when the cars are running north on such a road, their tendency is to run off the east side; but when the train is running south, their tendency is to run off on the west side of the track, i.e., always on the right-hand side. Whether the road be one mile or one hundred miles in length, the effect of diurnal rotation is the same; and the tendency to run off as you cross a given parallel at a stated rate of speed is the same, whether the road be long or short, the tendency to fly the track being in proportion to the speed of the trains, and not at all in proportion to the length of the road." Now, this article is quoted by many scientific authorities. It goes the yearly round of papers and periodicals. Is it true? To prove or disprove it, I have sent out a circular letter, to get from those familiar with railroads the facts on the subject. If it is true on a single-track road running north and south, with the same number of trains passing each way, the rails, and flanges of cars, not turned, would be equally worn. On double-track, the east rail north-bound, and west rail south-bound, would be most worn. Cars that were not turned would have their wheels and flanges equally worn; but locomotives, if "the tendency is always to the right," would have their right-hand flanges most worn. To facilitate the inquiry, I ask a list of questions. The questions are not asked for any personal advantage, but as of general scientific interest. 1. Do cars, when running north, have a tendency toward the east? 2. Do cars, when running south, have a tendency toward the west? 3. Have any instances come under your observation that indicate, by any wear of rails, of journals, of boxes, of flanges, or any part of a railway equipment, that "a train going north has a tendency to run off on the east side, but when the train is going south the tendency is to run off on the west side of the track"? 4. General remarks, with detailed description,— evidence *pro* or *con* from engines or rails.

JOHN C. GOODRIDGE, JR.

New York, Jan. 28.

Answers.

21. GLOBULAR LIGHTNING. — Governor Talmadge of Wisconsin lived in a two-story log-house on a level prairie near Fond du Lac, a short distance from a ridge of limestone that rose abruptly from the prairie. The upper story of the house had two rooms, with windows and doors forming a straight line through the house, and also an entry or hall between the rooms. One afternoon, when the windows and doors were open to allow a draught of air through the rooms, a ball, apparently a foot in diameter, floated slowly in one window, past Miss Talmadge, through the hall, and probably out of the other window, as the servant-girl ran screaming from that room. About the same time a barn near the house was struck and consumed. I could learn nothing further that was definite from those who saw the ball, when I reached the house.

T. MCDONOUGH.

Montclair, N.J., Jan. 24.

22. WASP-STINGS. — I have read with interest the items recently published in *Science* on this topic. Forty years ago, when a lad at school in Killingly, Conn. (in that part of the town at present known as Putnam Heights), I learned from schoolmates that any wasps could be handled without danger if one held his breath. I saw the experiment successfully made by many of my fellows, and ventured to make it myself with like results. Since that time scarcely a year has passed without my repeating the trial on wasps that have come in my way. I have never been stung except when I have forgotten myself, and allowed myself to inspire or expire the breath. Sometimes, after throwing the wasp violently away, I have been stung, because it had clung to my finger, and, not observing it, I had breathed. Ordinarily I notice after an experiment a slight feeling of numbness on the part of my hand where the wasp has attempted to sting me. I am accustomed to judge by this feeling whether the wasp was one of the stinging kind. As to the cause, I do not know of any. But many scientific persons have unscientifically refused to believe my statements, or to test them by experiment, because I could not answer their question, 'How do you account for it?' Whether the forced suspension of breathing paralyzes the nerves near the surface of the skin, — whether it stops the capillary circulation near the surface, — or whether its effect is something altogether different, I do not know. Nor do I see exactly how a paralysis of the superficial nerves, or an influence on the surface circulation, would prevent the poison from giving pain after commencing respiration again, provided that the wasp has succeeded in piercing the outer layer of the skin: for if the poison is exuded from the stinger, as I have sometimes seen it, it would act effectively upon removal of the paralysis when breathing is resumed. But my experience seems to lead to the conclusion that the poison does not penetrate at all during the suspension of the breath, but is left on the surface of the skin, and produces only the effect of a faint numbness after its effects begin to be felt through the outer coating. I do not take up this subject as one who has conducted any careful scientific experiments on it. My account of the matter may, however, help, like former articles in *Science*, in interesting experts in physiology to make genuine scientific experiments. One may hope that something important will be discovered in regard to the effect of forced suspension of the breath upon the nerves of feeling, the capillary circulation, or the resistance of the skin to penetration.

W. T. HARRIS.

Concord, Mass., Jan. 29.

23. DROPS OF WATER. — In answer to E. J. Pond's query in *Science* for Jan. 20, it seems to me that the phenomenon is explainable in the same way as the related phenomenon of drops of water on a hot stove; viz., rapid evaporation causes a layer of vapor to surround the drop, and this, by its repulsive expansion, keeps the globule of liquid from touching the hot metal in the one case, and the surface of the water in the other. The small drops that fall from the oar-blade will float a short time before calescing, even when no wind is blowing; the fall through the air apparently setting up evaporation enough to bear up the tiny globule. I have seen them at night, when the air was perfectly still, gleaming like seed-pearls in the moonlight. When the wind is strong, much larger drops will be supported because of the rapid evaporation.

C. M. WIRICK.

Metropolis, Ill., Jan. 24.